rule—no instrument has yet been devised which will invariably do this—but should not be too sensitive, or the record of important disturbances may be lost.

(4) The records should be capable of easy and rapid reproduction.

Of the instruments which have been designed or suggested for this purpose, four types are in use to a greater or less extent.

The Wiechert so-called astatic pendulum is an inverted pendulum with a bob weighing more than a ton, kept in position by two springs, and provided with an ingenious system of air-damping of its vibrations. This instrument has been recommended for general use, because its supposed astatic nature is believed to make it record the movement of the soil in an accurate manner; as already pointed out, this condition is immaterial, and, moreover, cannot be completely fulfilled. The instrument is undoubtedly a fine one, and gives valuable records, but its proper place is in an observatory specially devoted to seismology; for general use it is too heavy, requires too much attention, and gives records which are not adapted for ready and rapid reproduction.

The Rebeur-Ehlert instrument is a horizontal pendulum, of the form devised by v. Rebeur-Paschwitz, combined with a recording arrangement devised by Prof. Ehlert. This instrument is an extremely sensitive one, and there seems to be none better for recording small disturbances; in the case of large earthquakes the record is apt to be lost. The record is photographic, and the seismograms are readily reproduced by photography. Its cost of maintenance and too great sensitiveness are the points in which it fails to meet the requirements of an instrument for general adoption.

The so-called Omori pendulum is a horizontal pendulum presenting no special peculiarities, and is a modification, in details only, of a type of instrument in very general use. It fulfils all the first three requirements, being moderate in size, needing little attention, and gives good records, easy of interpretation and measurement. It fails in the fourth requirement only; the record, being taken on smoked paper, is not readily reproduced by photography, and is on too small a scale to obviate the risk of introducing error when copied by tracing.

The fourth type of instrument is the Milne pendulum, a horizontal pendulum with photographic record on a principle quite different from that adopted in any other instrument. This fulfils all the first three requirements, and the fourth too; the seismograms are easily reproduced by contact printing on to the same photographic paper that is used for recording, and the copies are practically as serviceable as the originals. This alone, if the instrument had few other merits, would mean much; but in addition to this I have found its seismograms the most convenient of any for determining the exact time of any point on the record, and had it not been for the general adoption of this type of instrument, and the ease with which its records can be reproduced, a considerable part of what seismological work I have been able to do could not have been attempted. The only improvement I have ever desired to see is an increase in the rate of movement of the recording surface, and this has now been introduced. I have examined and studied hundreds of records of this instrument from different stations; from Victoria, Toronto, Cape Town, Bidston, Paisley, and many other places, its records are consistently good; at a few stations, whether from a defect in the particular instrument, a want of proper adjustment, or, more probably, something in the foundations or the subsoil, its records are less satisfactory, but from none do they seem to be so bad as at Strassburg; having never seen a seismogram of that instrument—it is not easy to get copies from Strassburg—it is impossible to hazard a suggestion of the reason for the failure of the instrument at this station.

No one would wish to see one pattern of instrument adopted to the exclusion of all others, nor has it ever been pretended that the pattern adopted by the organisation which has grown up under the auspices of the British Association is faultless; but for the purpose of securing a large number of records for comparison with each other, and thereby determining the rate of transmission of earthquakes across, through, and around the earth, it is no more faulty than any other pattern, and has one crowning merit which they do not possess. Can it be to this, to the ease of reproduction of its records, which renders unnecessary the centralisation of seismological research, that we must attribute the continuous vilification of a valuable type of instrument?

R. D. OLDHAM.

An Early Acoustical Analogue of Michelson's Echelon Grating.

IN the "Œuvres complètes." of Christiaan Huygens (tome x., p. 571) occurs the note given below. It was destined for Ph. de la Hire, and of date November, 1693. Huygens's remarkable observation and his ingenious explanation of the musical note produced by reflection from a large flight of steps of the noise of a fountain in the park of Chantilly will be read with interest also by those who, though having no ready access to the "Œuvres complètes," are still concerned with the (reflecting) echelon grating:—

"Je veux adjouter icy au sujet de la réflexion du son une observation assez singulière, que j'ay fait autrefois estant à la belle maison de Chantilly de la Cour où est la statue Equestre on descend avec un degré large de . . . marches dans le parterre ou il y a une fontaine de celles qu'on appelle gerbe d'eau, qui fait un bruit continuel. Quand on est descendu en bas et qu'on se tient entre le degré et la fontaine on entend du costé du degré une résonance qui a un certain ton de musique qui dure continuellement, tant que la gerbe jette de l'eau. On ne scavoit pas d'où venoit ce son ou en disoit des causes peu vraisemblables ce qui me donna envie d'en chercher une meilleure. Je trouvay bientost qu'il procédoit de la réflexion du bruit de la fontaine contre les pierres du degré. Car comme tout son, ou plustost bruit, réitéré par des intervalles égaux et très petits fait un son de musique, et que la longueur d'un tuyau d'orgue détermine le ton qu'il a par sa longueur par ce que les battements de l'air arrivent également dans les petits intervalles de temps que ses ondoiements emploient à faire deux fois la longueur du tuyau sçavoir quand il est fermé par le bout, ainsi je concevois que chaque bruit tant soit peu distingué qui venoit de la fontaine, estant réflechi contre les marches du degré, devoit arriver à l'oreille de chacune d'autant plus tard qu'elle estoit plus éloignée, et cela par des différences de temps justement égales à celuy que les ondoiements de l'air employent a aller et venir autant qu'estoit la largeur d'une marche. Ayant mesuré cette largeur qui estoit de 17 pouces, je fis un rouleau de papier qui avoit cette longueur, et je trouvai qu'il avoit le mesme ton qu'on entendoit au bas du degré.

"Je trouvay comme j'ay dit que la gerbe n'allant point l'on cessoit d'entendre ce ton. Et aiant eu occasion d'aller a Chantilly pendant l'hyver, qu'il estoit tombé beaucoup de niège qui ostoit la forme aux marches, je remarquay que on n'entendoit rien quoyque la gerbe allast et fit du bruit à l'ordinaire."

A slight confusion is caused by Huygens's first referring in his note (apparently only drafted) to a closed organ-pipe and later to an open one. Taking a pouce=2·7 cm., the depth of the steps becomes $17 \times 2{\cdot}7 = 45{\cdot}9$ cm. At 10° C., the corresponding sound of about 368 vibrations per second would be given by an open pipe of 46 cm.

The effect of gratings on impulsive motion of light is now well understood, thanks to the labours of Lord Rayleigh, Gouy, Schuster, and others. It remains interesting, however, to contrast the opinion concerning the supposed regularity of white light, held by some high authorities before these discussions, with Huygens's statement that the regularity in the nature of the sound which he observed has been impressed upon it by outside influence.

P. ZEEMAN.

Amsterdam, January 6.

The Inheritance of "Acquired" Characters.

MAY I ask for information upon the interpretation of two sets of facts?

(1) Prof. Henslow states that the garden parsnip "known in the trade as 'The Student' was raised from seed of the wild plant by Prof. J. Buckman in 1847 at the Agricultural College, Cirencester," and also that M.

Carrière "raised several garden forms" of radish "of various colours from the seed of the wild species (*R. raphanistrum*); and found that they produced the long form in a light soil, and the turnip-rooted form in a stiff soil. A similar result has occurred with carrots. By selecting seed from plants having the best formed roots, these" (characters) "have become fixed and hereditary" ("How to Study Wild Flowers," 1902).

(2) I was delighted in the early summer by the marvellous skill and intelligence exhibited by some collies in the annual sheep-dog trials, which reveal apparently much more than the results of individual training. I have lately seen a pack of hounds streaming over the same country after a fox. The hound (*triste lupus in stabulis*) would make an indifferent sheep-dog, and the master who brought a pack of collies to a meet would provide some novel sport for the field. The collie is trained individually, but he has an inherent, if not inherited, aptitude, just as the foolishly good-natured hound puppy who is "put out to nurse" in his earlier days readily learns his work when he joins the pack. Further than this, an ordinary dog-show displays group peculiarities in different types of dog. The fox-terriers snarl savagely at each other, the greyhounds and their allies bark and yelp continuously, and appear as though on the verge of neurotic insanity, while the foxhounds lie and appear to drowse silently with a well-bred air of tolerant boredom that forms a curious contrast to the howling multitudes around them. Yet they are all dogs, and have reached their typical specialisation by characters acquired in some way.

If we are forbidden to believe that acquired characters are hereditary, what is the explanation of the seed of the "student" parsnip and the "turnip" radish coming true to type, and why does a collie drive sheep and a hound give tongue at the scent of a fox? Is it suggested that in the "germ plasm" of the wild dog all these special qualities are already involved, just as the digestive peptones gathered functionally and localised in the leaves of Dionæa and Drosera are found wandering aimlessly and to no purpose in some fruit trees? If so, what is the nature of the directive impulse that localises these characteristics in hound, collie, Drosera, and radish immediately fertilisation takes place? And again, how does the "peppered moth" contrive to appear in the black country hatched with sooty wings that harmonise with the now smoke-stained bark whereon he must rest? The whole subject of mimicry seems to be involved, and if your reviewer is right (NATURE, January 2, p. 193) in noting with "a sense of weariness mingled with surprise the appearance of a book on the transmission of acquired characters," it is quite certain that the "isolated biologists, and whole hosts of medical men who still hold the belief that acquired characters are transmitted" would regard it as a great boon if he would tell those who "make him tired" what are the conclusions apparently already established by "the modern, and still infant, science of heredity" that will satisfactorily account for such facts as I have ventured to state.

It will hardly do to say that in one sense the problem is "as unreal as the question of the apple dumpling which puzzled one of the Georges, because the characters of an organism do not get into its germ-cells any more than the apple gets into its crust, for both the germ-cells and the apple were there all the time." "One of the Georges" would doubtless have been grateful for a little culinary instruction, just as "whole hosts" of somewhat puzzled people with open minds would be honestly sincerely grateful for a definite explanation from "the infant science of heredity" as to how the sheep-driving impulse really got into the sheep-dog. For "nature" is more luminous than a text-book. E. C. SPICER.

Waterstock, Oxford, January 3.

The Diamantiferous Rock of Kimberley.

MY friend, Dr. Hatch, is not quite correct in stating (January 9, p. 225) that I was led to dissent from the late Prof. Carvill Lewis's view that the diamantiferous rock of Kimberley was a volcanic peridotite "by a microscopic examination made in 1899 of specimens from the Newlands Mines" (Proc. Roy. Soc., lxv., 1899, p. 223). Four years earlier I expressed the opinion that this rock was a breccia, and that the diamonds, with other conspicuous minerals, were not formed *in situ* (*Geol. Mag.*, 1895, p. 500). This belief was strengthened rather than shaken by editing Prof. Carvill Lewis's notes and examining his specimens ("The Genesis of the Diamond"), and was expressed yet more decidedly later in 1897 after examining another series of specimens from Kimberley (see *Geol. Mag.*, 1897, p. 501). To discuss the "magma" and "concretion" hypothesis would be out of place here, but elsewhere I may have something to say on those subjects.

T. G. BONNEY.

Musical Sands.

MR. CARUS-WILSON'S failure (January 9, p. 222) to obtain sounds from "millet seed" sand of highly spherical grains puts a difficulty in the way of the suggestion made in "Sound" by Poynting and Thomson, though I do not think that it finally disposes of it.

I have not been able to follow the friction explanation as given by Mr. Carus-Wilson (NATURE, August 6, 1891), and I write in the hope that he may give more detail as to the moving system which produces the musical note. It appears probable that the musical sounds excited in a body by friction are due to the natural vibrations of that body. Obviously the grains of sand are far too small to give the notes heard. I suppose that the fundamental period is of the order of the time taken by an elastic wave to travel half round the grain. With elastic moduli of the order 10^{11} and density $2\frac{1}{2}$, the fundamental frequency would be not less than 10^6. What system does the friction set in vibration? J. H. POYNTING.

The University, Birmingham, January 11.

IN NATURE of January 9 (p. 222) Mr. Carus-Wilson's letter asks for further details of the "singing sands" that I exhibited to the Physical Society. I am able to give the mineralogical description, by Mr. A. J. Maslen, of the Maine sand from Mareen's beach, near Small Point, at the entrance of the Kennebec River.

A subangular sand very free from *very* small grains. Clean.

Quartz. Principal constituent. As perfectly colourless grains showing conchoidal fracture (rock-crystal) and as more or less coloured grains of quartzite.

Muscovite Mica. Small quantity. Flakes.

Glauconite. Dark green grains, many of fairly large size. Almost black.

Topaz (?) Square pieces due to cleavage. Yellow.

Opaque white substance. (Felspar?)

Magnetite. Small grains. Rare.

The other specimens of sands were very similar to tha from Maine. SIDNEY SKINNER.

South-Western Polytechnic, Chelsea, S.W., January 13.

Intensity of Spectrum Lines.

VERY little attention has been paid in the past to the accurate measurement of the optical intensity of spectral lines in vacuum tubes under different conditions, probably on account of the considerable experimental difficulties. Hence I may, perhaps, be allowed to indicate a relation I have obtained between the optical intensity, current strength, and pressure of the glowing gas. Throughout the whole experimental range, using the so-called "electrodeless" tube—with wholly external mercury electrodes, when the current is of a slowly oscillating character—the optical intensity, with an end-on tube, is accurately proportional to the readings of a thermo-galvanometer in series, and over a more limited range of measurement, at constant current, is inversely proportional to the pressure of the gas.

In other words, the intensity is proportional to $\lambda \int c^2 \delta t$, where λ is the *mean free path*.

Measurements on the monatomic gases are now in progress, and it is intended later to investigate the influence of temperature. A. D. COWPER.

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